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GENERAL REVIEWS AND SUMMARIES

SENSATION (GENERAL)

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Minkowski (5) denies the validity of Müller's law of specific sense energies. He finds that the law is inconsistent with the concept of the adequate stimulus, and that it fails also to allow, if taken in strictness, the attribution of qualitative differences to the world of objects. Moreover, he finds Müller inconsistent in applying the law to the modalities of sense while conceding that stimulus may condition the individual qualities (colors, tones, etc.) within a single modality. Minkowski brings the argument from phylogeny to show that nervous system and end-organ have grown up under the influence of the environment and that they have been moulded in conformity to the properties of stimulus. The specificity of response he refers to Nagel's "specific disposition" which tunes the sensory apparatus, from the start, to a particular form of stimulation. A. Schönberg (6) thinks, on the contrary, that the doctrine of specific energies may be retained by assuming a relatively small number of nervous elements which respond differently under different intensities of stimulus. From the observation that strong taste stimuli often yield not one but two or three taste qualities, he draws the wide inference that "with every quantitative change of stimulus there is correlated a qualitative change in sensation." Schönberg cites other sense modalities, too; but he quite fails to support his generalization. Structures found within several of the sense-organs and known to the histologists as "secondary sense-cells" are thought by Botezat (1) to be of a glandular nature and to facilitate, by their

secretions, the excitatory functions of the end-organ. Botezat calls them "sensory gland-cells" (*Sinnesdrüsenzellen*). He includes the auditory cells of the labyrinth, rod-cells of the taste-buds, the cells in the rod-cone layer of the retina, and other similar structures. Upon his view, the stimulus sets these cells secreting, and their products act chemically upon the nervous substance. By treating the stimulating agents in an animal's environment under the principle of the parallelogram of forces, Szymanski (7) seeks to work out the relative physiological values of light, heat, etc., as these stimuli simultaneously affect the organism. The rate and direction of movement under combined stimuli are regarded by his method as resultants to be factored into stimulus-moments. From a brilliant series of experiments under the method of training (*Dressurmethode*), Kalischer (3) concludes (1) that the labyrinth is not an organ of analysis, (2) that the vestibule possesses auditory functions, and (3) that the cortex is not the sole seat of auditory and visual processes. Kalischer trained dogs to associate the taking of food with certain tones, odors, and colors. When the association had become fixed, the animal suffered an operation (such as the removal of the labyrinth or of the temporal or occipital lobes). From the subsequent retention or loss of the trained response the experimenter drew his inferences regarding the psychophysical processes involved. The brief account of the experiments leaves one in doubt whether sufficient precautions were taken against secondary cues. The method of training has for some time been used in this country by S. I. Franz (2) in his study of cerebral functions.

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VISION—GENERAL PHENOMENA

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In his experiments upon the retinas of frogs, Bauer (1) found strong evidence that an assimilating as well as a dissimilating process takes place in the visual purple in daylight vision and that the assimilating process increases with increasing intensity of light up to a certain maximum intensity. Upon first submitting the purple to strong light, it pales, but gradually adaptation takes place and the assimilating process overtakes and often exceeds the dissimilating process. In the latter case the purple returns to the original dark color. Absence of change in the color of the purple does not mean that there is no retinal process in progress, but that the assimilating process is equal to the dissimilating. These facts seem to support the theory of Hering as against that of v. Kries. The visual purple of the rods not only functions in daylight vision but it is only then that it reaches its full activity. Sivén (17) thinks the following facts point to the possible functioning of the rods for blue-violet perception: first, the spectrum at low intensities is not colorless but bluish; second, the Purkinje phenomenon takes place in the region of the rods; third, violet blindness during santonin poisoning,—since santonin affects the rods; fourth, the yellow perception during jaundice caused by affection of the rods; fifth, blue-blindness during hemeralopia; sixth, the results of the experiments of Hess on the color vision of night and day animals. The experiments which Brückner made upon the blind spot,[1] in which he found a contrast effect of the surrounding field upon the blind spot, leads him to conclude (3) that the physiological processes underlying the phenomenon of contrast take place not in the retina, but in the corpus geniculatum externum or in the visual cortex. In a preliminary report of experiments upon after images, Ferree and Rand (9) state that the results so far obtained indicate that the influence of brightness upon color excitation takes place posterior to the level usually ascribed to the paired processes.

The experiment of S. P. Thompson,[2] in which a sensation of light is caused by subjecting the head to the influence of a magnetic field,

[1] Brüchner, A. "Ueber die Sichtbarkeit des blinden Fleckes." *Arch. f. d. ges. Physiol.* (Pflüger), 1911, 136, 610–658.

[2] Thompson, S. P. "A Physiological Effect of an Alternating Magnetic Field." *Proc. Roy. Soc.*, 1910, 82, 396–398.

was repeated by Dunlap (8) in order to determine if the phenomenon was not "due to idio-retinal light under the suggestion of the hum of the coil due to the alternating current." The transformer was placed on a table near the coil "so that the loud noise of the former completely drowned the hum of the latter" and the sensation still persisted. Dunlap therefore concludes that "the phenomenon was really a matter of visual sensation." Of special interest is the fact that the sensation is strongest when the head is so turned in the magnetic field that the general direction of the optic pathway is parallel to the lines of force. Dunlap thinks rather that the alternating current alternately intensifies and inhibits a process already in progress, such as idio-retinal light, rather than that it arouses a visual sensation, but he hesitates to decide definitely from the present data. Swinton (19) some fifteen years ago observed this phenomenon of visual sensation due to the effect of the electric current. His method was to employ a continuous-current magneto generator, one terminal of which was held in one hand, while a wire from the other together with a wet sponge was held by the other hand to the side of the head. The frequency of the flicker increased with the speed of the generator. Swinton adds that this method also precludes the possibility of suggestion being the cause of the flicker.

One of the reasons given by Edridge-Green[1] in explanation of the fact that Lord Rayleigh's threshold for change in the hue of yellow (*D*) light was so much lower than the threshold he obtained by his method, namely, because of the admixture of white light in Rayleigh's experiment, has been proven false by a series of experiments conducted by Watson (21). By means of the Abney double spectrum apparatus, two fields of light of the same wave-length were projected side by side on a magnesium carbonate screen and one field changed in wave-length until the difference in hue was quite distinct. A difference of 1.4 $\mu\mu$ was detected, while Edridge-Green's monochromatic patch measured 4.5 $\mu\mu$. Watson then found that additions of small amounts of white light made no change in the threshold. The difference in the results of Edridge-Green and Rayleigh seems to be caused by a difference in the method of presenting the change, namely, whether two monochromatic patches are used or a single patch in which the hue changes gradually from one side to the other.

A comparison of the effect of the exposure of the eye for ten seconds to a continuous stimulation of light with that of a like exposure to an

[1] Edridge-Green, F. W. "The Discrimination of Colour." *Proc. Roy. Soc.*, 1911, B. 84, 116–118.

intermittent stimulation, upon the subsequent process of adaptation, was made by Schneider (16). The rate of alternation of light and darkness was varied as well as the relative amount of light to darkness in a given period. The intensity of the continuous light was so chosen that the brightness produced was equal, according to the Talbot law, to the brightness of the compared intermittent light. It was found that the two adaptation curves approached one another under the following conditions: first, by increasing the rapidity of alternation of the intermittent light; second, by increasing the difference between the exposure time for light and for darkness; third, by increasing the mean intensity of the intermittent light. These results were considered the more significant in that the above conditions are the same as those found by Marbe[1] to be most conducive to an elimination of flicker.

Behr (2), in his article upon the relation of dark adaptation to certain pathological conditions, includes the interesting fact that if one eye is exposed to light while the other is being dark adapted, the latter after three quarters of an hour is only half as sensitive to light as it would have been if both eyes had been dark adapted. He concludes from this that the action of the rods and the visual purple are in direct relation to a higher process. Dufour (4) has made some observations upon the after-images of motion of the Plateau spiral as well as upon those of the motion of translation. He found that if the motion was observed monocularly a negative after-image could be obtained from the eye which had been closed.

A preliminary study has been made by v. Liebermann (11) of the rate of rotation necessary for the fusion of different pairs of colors. No definite results were obtained, but the methods used and the precautions observed, as well as some of the difficulties of the problem discovered, are of interest. Dufour and Verain (7) explain a simple method of obtaining the threshold for the perception of flicker, namely, by placing a disk of n white and black sectors upon a larger disk of $n + 1$ white and black sectors. Dufour (6) has observed the same laws of fusion, flicker, etc., to hold in movements of translation of bands as in rotatory movement of disks. He also describes (5) a simple device for obtaining these movements of translation.

A method of obtaining the time that it takes for a sensation of light to reach its maximum brightness has been devised by Stigler (18). The instrument, which he calls a chronophotometer, permits

[1] Marbe, K. "Tatsachen und Theorien des Talbot'schen Gesetzes." *Arch. f. d. ges. Physiol.* (Pflüger), 1903, **97**, 335–393.

the successive exposure of two neighboring fields of the same or different intensities of light and the regulation and measurement of the exposure time as well as the interval between the two exposures. If the first sensation does not reach its maximum intensity at the time of exposure of the second stimulus, it will never reach it in that part of the field adjacent to the second field, owing to the contrasting effect of the latter. By regulating the exposure times and the intervals between the two stimulations and comparing the intensity of all parts of the two fields, the approximate time necessary for the first sensation to reach its maximum intensity may be ascertained.

The phenomenon of brightness contrast has been used by Révész (15) as a basis for a new method of measuring the brightness of colors of different hues. The maximum saturation of a color can be obtained by gradually increasing the brightness of a contrast-producing field until the white (W-Valenz) of the color is entirely compensated. It follows that if two colors, for example red and green, both reach their maximum of saturation with the same increase of brightness of the contrasting field, they may be considered to contain the same amount of white. The relative amount of white, which different colors were found to contain by this method, coincides with the relative brightness of the colors as determined both by the direct method of measuring brightness at the fovea, and by the method of indirect vision. The author finds theoretical support for his method in G. E. Müller's theory of vision.

One of the most suggestive books of the year is that by Katz (10). He has given us the result of five years investigations of the various characteristics of colors and especially of those changes in these characteristics brought about by experience. He has displayed much ingenuity in the methods of his countless experiments and in the variation of the conditions, as well as a keen power of analysis and an appreciation of the relation of his results to the more general problems of vision. It is only possible here to mention some of the most important facts. As regards the localization in space, idioretinal light is characterized by an indefiniteness of localization. The more vivid (*eindringlich*) an after-image is, the nearer it appears. What is meant by vividness of colors may be understood if one fixates the middle of a larger white surface; it will then be observed that the color gradually lessens in vividness towards the periphery. Idioretinal light shows the lowest degree of vividness. The darker colors seem nearer than the lighter.

There are three forms in which colors may appear: first, as sur-

face colors (*Oberflächenfarben*), that is, the surface of an object seems to have a certain color; second, as surfaces (*Flächenfarben*), that is, the colors are perceived in two dimensions and without any reference to an object; third, as transparent colors in three dimensions. The last two may be reduced to surface colors by observing them through the small aperture of a screen. It is in perceiving colors as surface colors that the results of experience play a rôle. It is then that we abstract as far as possible from the existing conditions of illumination and see the color as it would appear under normal conditions. The conditions may be considered normal when the objects are seen most sharply defined and outlined. These conditions may be obtained in the open air and under a slightly clouded sky.

If we look at a gray disk placed some distance behind a revolving black episkotister through a small hole in a screen in front of the instrument, and notice that the episkotister is cutting off some of the light, we abstract from its effect, and see the gray in its normal color, that is, brighter than it appears when the conditions behind the screen are concealed from us. Vividness, however, is not affected by psychological factors, for vividness depends upon the absolute amount of light falling on the retina. Neither do psychological factors influence the difference threshold for brightness nor the absolute threshold for the normal adapted eye. But the Talbot law does not hold for brightness under the above conditions, the variations from the law becoming greater the more vivid the psychological factors become. As might be supposed from the above, the Talbot law does hold for the vividness of colors.

If it is seen that a paper is darkened by a shadow, it appears lighter than when this fact is concealed from the observer. The relative brightening of the paper by psychological factors is greater the deeper the shadow. Individual differences are greater when the psychological factors are most influential. Katz does not call these phenomena illusions, since attending to them does not entirely eliminate them. The longer one observes, the stronger is the effect of the psychological factors, but even in a very short exposure they are influential. Hering's memory-color (*Gedächtnisfarben*) theory only applies to objects with which we are very familiar. Hering explains phenomena similar to the above through adaptation and other physiological causes. Such physiological explanations are considered by Katz secondary to a psychological explanation. Also the fact that colored papers illuminated by colored light are seen in

their original colors is explained by Katz through psychological factors as against Hering's physiological explanation. On the other hand, Katz's results agree with Hering's, that contrast depends upon the intensity and quality of the retinal processes and not upon psychological factors. From the experiments on dark adaptation, Katz comes to the conclusion that the psychological factors only alter those sensations depending upon processes in the cones. Many interesting references are also made to the influence of psychological factors similar to the above in the effect of works of art.

Tucker (20) has made a series of tests on the color vision of sixty-three girls and sixty-four boys of the English schools, in order to compare their color sense with that of primitive peoples. In the color discrimination test with Holmgren's wools, in the case of all the children "blue and violet tend to be confused. Then the images with green extend their range and finally those of pink, red and yellow." In the color nomenclature test, similar mistakes were made. The names included a wide range of hues. These results coincide with those obtained with primitive peoples. Tests for the threshold for red, yellow, and blue were also made and it was found that the threshold for colors rose as the age decreased, but that the ratio of the threshold of one color to the other remained unchanged both in the tests on children and those on adults. Since these quantitative results show that the relative threshold for blue did not change, and the qualitative results, that the children made the same mistakes in color discrimination as primitive races, the author thinks that the latter's confusion of colors can hardly be explained satisfactorily through a weakness for blue due to a greater yellow pigmentation of the macula lutea among dark-skinned races. Her conclusions are rather that there are two causes for the peculiarity in color confusion among primitive people; the one, psychological, "depending on the stage of the development of the powers of observation and thought leading to mistakes similar to those made by European people," the other physiological, depending on the stage of the development of the sense organ.

Leob (14) in his experiments upon the memory for colors used the Asher color mixture apparatus and allowed from five minutes to several days to intervene between the exposure of the color and the reproduction. Loeb concludes from the fact that the m.v. for the eleven trials made in the reproduction test for each color was less for blue and yellow than for red and green, that the precision of reproduction, and with it the actual memory for colors for the

former pair, is greater than for the latter, although the colors reproduced were further from the original in the case of blue and yellow than in that of red and green. This latter result he considers to be caused by the conditions of his method and not to be contradictory to the results of L. v. Kries and E. Schottelius[1] whose order of accuracy of reproduction was the same as his order of precision. He is, however, hardly justified in deducing an accuracy of memory from a small m.v.

Luckiesh (12) has measured the difference in visual acuity in monochromatic light and in light having an extended spectrum. To quote from the author's summary in this journal:[2]

> "The green line of the mercury vapor spectrum was isolated and used as a monochromatic source. This line was matched in hue by light having an extended spectrum obtained by filters used with the tungsten lamp. These two green colors could easily be matched for brightness without any of the difficulties which would obtain if they differed in hue.
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> "A printed page of type of such size that at a distance of 1 meter it was just readable in monochromatic light, was arranged in a photometer so that two adjacent patches were illuminated respectively by the two green lights of different spectral character. For the same ease in reading it was found that the illumination having an extended spectrum must be increased 75 per cent. over the monochromatic illumination. This result was substantiated by other experiments. Later the Ives[3] acuity test object was used and by this more sensitive method it was found that for two observers the illumination having an extended spectrum must be five times greater than that of the monochromatic illumination for the same visual acuity. Another observer required an increase of only 33 per cent. It was shown that there was no movement of the pupil when alternately subjected to the two lights."

Luckiesh (13) has also investigated the relation between visual acuity and wave-length. The influence of brightness differences was as far as possible eliminated. He found that "the extremes of the visible spectrum show a lower defining power than the middle region, the maximum acuity appearing to be in the yellow region."

Two essays, one by Woodworth (22), and the other by Yerkes (23), embodying the facts of the sensation of light most important for illuminating engineering, have appeared.

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VISION—PERIPHERAL, FOVEAL, ETC.

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The year's work in this field is comprised in four articles.

A. Pick (4) gives a clinical report on cases of functional narrowing of the visual field. He concludes, in contradiction to the commonly accepted view, that in these cases the patient is just as much aware of his defect as is the patient who suffers the contraction from organic causes. Two circumstances he thinks have led to the false conclusion by previous writers. (1) The patient has not been carefully questioned with regard to what he sees. (2) The examiner has been influenced by certain objective signs which have not been studied with sufficient care. For example, it has been claimed that no disturbance of orientation is suffered. The patient must, therefore, not be aware of the defect in his visual field. But Pick and many others have found that disturbances in orientation do occur. They last, however, only a short time after the contraction has begun. The patient soon adapts to his changed condition of seeing and recovers his power of orientation. This is what happens in every case of sudden loss of visual acuity. It also happens when the field is narrowed from organic causes. Such evidence therefore, argues Pick, should not lead to the conclusion that the patient suffering from functional contraction is less aware of his defect than the patient with organic disease.

Pick supports his position by the direct testimony of his patients. He wishes, moreover, to emphasize the importance of carefully questioning the patient in making diagnoses. The testimony of the patient is necessary to diagnosis and it can not be assumed that he can and will tell of his own accord all the diagnostician needs to know.

Edridge-Green (2) describes some visual phenomena connected with the yellow spot. The article consists of a statement of the observations of previous writers on the following points supplemented by some observations of his own: various appearances in the field of vision due to peculiarities of the yellow spot; entoptic appearance of the yellow spot and the blood vessels of the retina; currents seen in the field of vision not due to circulation; and appearances due to the pigment cells of the retina. The third of these topics alone contains sufficient new material to warrant mention in this review. This topic is of particular importance to Edridge-Green

because of its bearing on his theory of vision. He assumes that all visual sensations are caused by the decomposition of the visual purple. This decomposition sets up electrical impulses which travel along the optic nerve to the brain. These impulses he believes are wave-like and periodic in nature. Each wave-length of light sets up an impulse different from that set up by every other wave-length. A physiological basis is thus laid for the different visual qualities. The visual purple is generated in the rods, hence there is none in the fovea. But it is necessary to foveal vision hence it must be supplied from the extra-foveal retina. It is the streaming of the visual purple from the peripheral retina to the fovea that constitutes, he thinks, the currents seen in the field of vision not due to circulation. He is able to see these currents under the following conditions: with one eye partially covered; with one or both eyes open at full illumination; with the eyes open in the dark-room; with a field of vision given by looking through a yellow-green glass; and with an intermittent stimulation by light produced by rotating a disk composed of white and black sectors. There are four main stream channels which are fixed in position. These four channels end in the fovea and form a figure closely resembling the written letter X. On examining the retina of a monkey he finds four shallow channels leading to the fovea which correspond roughly to the stream channels seen by him entoptically. Between the fixed stream channels indefinite streaming movements are observed.

The streaming has a characteristic effect on visual sensation. The stream currents carry the visual quality, color and brightness, of the region from which they come into the after-image. They also tend to move the after-image towards the center of the field of vision.[1]

[1] Edridge-Green seems inclined to identify this phenomenon with the streaming phenomenon described by the writer in 1908. The writer is strongly impelled to question the propriety of this identification because of the obvious disagreement of the phenomena in so many important particulars. (1) Even a casual comparison of the drawings representing the two phenomena shows many differences that are characteristic and essential. In fact not even a general similarity is found between the stream patterns and the appearance and behavior of the streaming material in the two cases. (2) The descriptions of the phenomenon given in the two cases are even more incompatible. If there is one thing above another that is characteristic of the "streaming phenomenon" as it was observed and described by the writer, it is just the absence of any fixed channel or path of movement. There is a general tendency for the streams to move towards the center of the field of vision but that movement may occur along any possible meridian in the field of vision. Very frequently also the stream is deflected from its course before it reaches the center of the field of vision or even that

Haycraft (3) reports work on the color sensitivity of the retina immediately surrounding the blind spot. In mapping the blind spot with a colored stimulus in 1907, he noticed that when the stimulus fell just within the blind spot a slight movement caused it to be seen as gray while a greater movement caused it to be seen in its proper color. This led him to investigate the relative sensitivity of the margin of the blind spot to colored and to colorless light.[1] The investigation was conducted by means of a scotometer. The scotometer consists of a head-rest, a projection-screen, and a supporting base. The projection-screen is provided with a fixation point, a movable stimulus, and a frame fastened immediately behind the movable stimulus to hold the paper on which the outline of the blind spot is to be traced. The movable stimulus is fastened on the front end of a plunger which, when pushed in, punches a hole in the paper in the frame behind. A line connecting these holes marks the outline of the blind spot. The stimulus is moved by means of two screws one of which gives it a motion in the vertical plane and the other in the horizontal plane. By means of this adjustment the position of the stimulus can be changed by small and definite amounts, a feature of particular advantage in the technique of the problem. Red, green, blue, yellow, and gray stimuli

part of the field corresponding to the "external fovea." This deflection is often traceable to an involuntary eye-movement and can generally be caused by a sharp voluntary movement executed at the right time. Space will not be taken here to enumerate other numerous and important points of difference. The points of similarity can be pointed out more briefly. Both are subjective movement phenomena not caused by the circulation of the blood, and the tendency of movement in both cases is towards the center of the field of vision. (3) Characteristic differences are also found in the effect on visual sensation. Edridge-Green says the currents described by him carry the visual quality, color and brightness, of the region from which they come into the after-image. They also tend to move the after-image towards the center of the field of vision. No further details are given. If one be permitted to infer details, it is obvious that the effect of streaming on the fluctuation of after-images described by the writer could not be compatible with a streaming system in which the distinctive and definite streaming is limited to four narrow channels. The writer is forced to conclude, then, that either Edridge-Green and he have not observed the same phenomenon, or that they have differed widely in their descriptions of its essential features.

[1] That there is a color-blind area around the blind spot has been mentioned by Johansson (*Upsala Läkareförenings Förhandlingar*, 1884, 19, 491–493), Ovio (*Annali di Ottalmologia*, a, 1906, 36), and Polimanti (*Jour. de Psychol.*, 1908, 5, 298). That the order of loss of sensitivity in passing from the surrounding retina into the blind spot is the same as it is in passing from the center towards the periphery of the retina was mentioned by the present writer in a paper read before the meeting of experimental psychologists held at Princeton in April, 1909.

were used in mapping the blind spot. The brightness of the colors was in each case made equal to the brightness of the yellow by the method described by Abney (*Philos. Trans. of Royal Soc.*, 1886 and 1892). Several observers were used and some variation was found in the order of loss of sensitivity for the different observers. In Haycraft's own case, as the blind spot was approached from any direction, sensitivity was lost in the following order: red, green and yellow, blue, and gray. Using the same stimuli to map the sensitivity of the retina as a whole, he found the same order of loss of sensitivity as the stimuli were moved from the center towards the periphery of the retina.

A. Brückner (1) publishes concerning "Die Sichtbarkeit des blinden Fleckes." Both the experimentation as it is described in the article and the conclusions seem to the writer to be in some measure open to question. Extended criticism, however, will not be attempted in this brief review.

Three explanations have been given for the absence of a gap in the monocular field of regard: Weber's theory of shrinkage, DuBois-Reymond's and Volkmann's theories of associative filling-in, and Tschermak's theory of physiological induction. Brückner decides against the theories of shrinkage and associative filling-in, and accepts the theory of physiological induction. The gap is filled in either by irradiation or by simultaneous contrast. It is filled in most frequently by irradiation. This is why in ordinary monocular vision we are not conscious of the blind spot.

Brückner is not the first to claim that visual sensations may be referred to that part of the field of vision usually called the blind spot. Prior mention has been made by Purkinje, Heinrich Müller, Meissner, Aubert, Charpentier, Woinow, Helmholtz, Finkelstein, Zehender, Tschermak, Czermak and others. Brückner aims merely to verify the observations of his predecessors and to extend the conditions under which the phenomenon may be observed. (1) With the eye thoroughly dark-adapted the blind spot may be seen as a dark disk surrounded by a light halo, immediately following a quick pressure of the front of the eye-ball through the closed lids. The phenomenon lasts only a fraction of a second. This observation has been previously made by Aubert and Finkelstein. (2) The blind spot may be seen by the method used by Purkinje to demonstrate the *Aderfigur*. This observation has been made by Heinrich Müller, and Tschermak. (3) When one looks with one eye at a uniform field, for example, the sky at twilight, one

sees the blind spot as a dark disk surrounded by a light halo. Observations of this kind have been made by Helmholtz, Woinow, Zehender, Charpentier, and Tschermak. Brückner extends the observation to fields of white, of black, and of color. On white paper he sees the blind spot as a shadowy spot surrounded by a light halo; on black paper as a spot of more intensive blackness surrounded by a light halo; on a field formed by looking at a neutral surface through colored glass as a dark spot surrounded by a halo of the color complementary to the field. (4) When the field is formed of black and white paper with their line of junction passing vertically through the blind spot, the part of the field towards the center of the retina is seen as bulged out to fill the area of the blind spot. Thus when the white field is towards the center of the retina, the blind spot is filled in with white; and, commonly, when the black field is towards the center of the retina, the blind spot is filled in with black. In each case the bulging portion is surrounded by a halo of antagonistic quality. The bulging is due to irradiation from the stronger field and the halo is due to marginal contrast. In using colored fields he finds that the law of irradiation from the stronger field does not always hold. The weaker field, *i. e.*, the field beyond the blind spot, sometimes fills in the gap. (5) An after-image may be gotten of the visual quality filling in the blind spot. When one with a thoroughly dark-adapted eye looks for a moment at the sky at twilight and then closes the eye, he sees at first a black disk with a halo in a light field which soon gives way to a light disk with a black halo in a dark field. This observation has been made by Charpentier and Tschermak. (6) At the make and break of an electric current, sent through the head by means of two electrodes, one applied to the middle of the forehead and the other to the back of the neck, the blind spot is seen sometimes as a light and sometimes as a dark disk. This phenomenon has been reported by Tschermak and others.

In general Brückner is inclined to attribute the visibility of the blind spot with the dark-adapted eye to contrast, and with the light-adapted eye to irradiation. He gets into difficulty, however, in attempting to apply this principle of explanation in detail. (1) When the field is white and the blind spot is seen as a dark disk surrounded by a light halo on a white ground, he explains the quality of the disk as due to contrast from the surrounding field. For the halo he gives the rather remarkable explanation that it is due to marginal contrast between the two fields, one of which he has already explained as a contrast effect due to the other. When the field is black and the

blind spot is seen as a disk of more intensive blackness, he offers no explanation, yet as before the observation was made with a dark-adapted eye. When the field is colored the blind spot is almost invariably seen as a dark disk instead of in the color complementary to the field. The halo in this case is of a color complementary to the field. He attempts to explain the failure of the blind spot to appear in the complementary color as due to the extreme susceptibility of the blind spot to fatigue. The contrast color is of such short duration it can not be observed. This explanation, however, can hardly be accepted as satisfactory. In the first place no reason is given why the blind spot should fatigue so easily to color and apparently not at all, within the limits of the observation, to brightness; and in the second place, according to the writer's experience, the tendency of color contrast within limits is to grow with prolonged observation rather than to disappear. (2) In case the field was half white and half black with the line of junction passing vertically through the blind spot, the observation was also made with a dark-adapted eye. Yet in this case Brückner says the blind spot is filled out by irradiation from the stronger half of the field.

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VISION—COLOR DEFECTS

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It has long been known that red-green blindness is inheritable and that it occurs more frequently among men than among women. As early as 1876 Horner (6) formulated a law of descent, that color-blindness is ordinarily transmitted to males through unaffected females. In a number of recent books on heredity the attempt has been made to explain the transmission of color-blindness by Mendel's law. Bateson (1) considers color-blindness to be a sex-limited unit character, dominant in males and recessive in females. "Color-blindness is not, therefore, as might have been imagined, a condition

due to the omission of something from the total ingredients of the body, but is plainly the consequence of the addition of some factor absent from the normal. We can scarcely avoid the surmise that this added element has the power of paralyzing the color-sense, somewhat as nicotin-poisoning may do." Bateson's views find expression in the recent books of Punnett (9) and Doncaster (4). The latter puts the matter briefly as follows: "Color-blindness is dominant in males, but recessive in females, but at the same time an affected man transmits the 'factor' for color-blindness only to his daughter so that while his sons and their descendants are free, his grandsons through his daughters may be affected" (p. 84). Castle (2) and Davenport (3) also regard color-blindness as sex-limited in descent, but consider the defect to be caused by the absence of a "factor" necessary to normal vision, a position more consistent with psychological theories of color-vision, and apply to the problem the recent cytological theory of sex-chromosomes. Castle (p. 180) says: "A color-blind man does not transmit color-blindness to his sons, but only to his daughters, the daughters, however, are themselves normal provided the mother was; yet they transmit color-blindness to half their sons. A color-blind daughter may be produced, apparently, only by the marriage of a color-blind man with a woman who transmitted color-blindness, since the daughter to be color-blind must have received the character from both parents, whereas the color-blind son receives the character only from his mother. Color-blindness is apparently due to a defect in the germ-cell—absence of something normally associated there with an *X*-structure, which is represented twice in woman, once in man."

The assertion is often made that dichromates equal or surpass persons of normal color vision in discrimination of small differences in color tone. The only experimental work upon the question, that of Brodhun, showed the *deuteranope* tested (Brodhun) to be more sensitive to difference in color tone in the more refrangible part of the spectrum than were two normal observers (König and Uhthoff). This superiority was most marked in the region of the neutral band. Liebermann and Marx (8) report an experimental investigation of the question, with a *protanope* and a normal person as subjects. The Helmholtz color-mixing apparatus was used, and tests made with a longer list of lights than Brodhun tried, including among others a non-spectral purple formed by the mixture of red and blue light, which appeared colorless to the protanopic subject. The results showed the protanope clearly inferior to the normal subject in distinguishing differences in color quality, throughout the whole series

of colored lights, thus directly contradicting Brodhun's results. The authors do not think it justifiable to assume that this difference is to be accounted for by the difference between protanopes and deuteranopes.

Köllner (7) shows how acquired color-blindness may be distinguished from congenital protanopia and deuteranopia by psychological tests, without reference to its accompanying physiological symptoms —lowered visual acuity, abnormal condition of the retina, etc. Acquired red-green blindness, like congenital red-green blindness, is a two-color system with a neutral zone in the green-blue region. In the acquired form, however, we find no division into distinct groups like protanopia and deuteranopia without transition forms, but, rather, a fairly definite condition of dichromatic vision which appears regularly at a certain stage in progressive tabetic atrophy of the optic nerve and in chronic alcohol poisoning. It is, further, a quantitative reduction from the normal condition, with color-memories intact, rather than a qualitative simplification of normal color-vision with dichromatic color-memories. By means of Nagel's color-mixing apparatus and anomaloscope, Köllner finds that in acquired red-green blindness the subjects see colors at a very low saturation (as they would appear to a normal person through a thin white veil). As a result of this, it is especially difficult to determine exactly the limits of the neutral zone, faint colors from the warm end of the spectrum which would appear yellowish to the congenital color-blinds being seen as white by the acquired color-blinds. Moreover, the stage of the progressive defect can be determined by finding which colors are equivalent to white, since they fade out in the following order: green, yellow, red, blue. Ordinarily red appears of about the same brightness to the acquired color-blind as to a deuteranope, about the same amount of yellow being needed in both cases to make the red-yellow equation. In two out of about 100 cases, the subjects showed the protanopic lowered sensitiveness to the red end of the spectrum, but in both cases there is considerable reason for assuming that congenital protanopia existed before the acquired defect developed.

Hayes (5) examines the evidence for the common assumption that all typical cases of partial color-blindness are dichromates—see only yellow and blue—and presents the results of a series of experiments upon 19 new cases, one of whom is a woman color-blind in one eye only. He feels that theoretical bias has prejudiced the interpretation of the facts in many of the published articles on color-blindness; and upon the basis of his own historical and experimental work he concludes that dichromacy is not a typical but an extreme condition of partial color-blindness connected with normal vision by

a series of intermediate forms showing greater or less deficiency in red and green, but not totally lacking red and green sensations. Five lines of evidence are considered. (1) Opposed to the testimony of various color-blinds (Dalton, Pole, etc.) that they see only blue and yellow, Hayes presents the testimony of five of his subjects that red and green are specifically different color qualities from yellow and gray. This claim is further supported by a study of the color-confusions made by these observers. (2) Dichromates should accept mixtures of blue, yellow, black and white as matches for all colors. But when Hayes presented reds and greens to his observers under favorable conditions—high saturation, large area, bright illumination—no equations could be made, oftentimes, without the addition of red or green to the dichromate mixture. (3) Hayes presents no new data upon acquired color-blindness, and thinks it unsafe at present to claim analogies between the acquired and congenital forms. The findings of other investigators, however, raise a significant question, whose ultimate solution promises support to the thesis of his paper. If sensitivity to green may lapse before sensitivity to red is lost, and if transitional forms between trichromacy and dichromacy occur in acquired color-blindness, what theoretical warrant can there be for refusing to believe that an analogous series of transitional forms occurs in congenital color-blindness? (4) To meet the claim that colors appear to the partially color-blind as they do to normal persons stimulated in the blue-yellow zone of the eye, Hayes quotes Baird's conclusion that retinal function in the periphery lapses, when it does lapse, in a gradual and not in an abrupt fashion, a conclusion which supports his own conclusion. (5) A review of the experiments performed upon the 7 historical cases of monocular red-green blindness shows little evidence of strict dichromacy except in the case reported by von Hippel, while the new case extensively studied by Hayes gives undoubted evidence of the appreciation of green as a distinct color quality. In general, then, as there seems to be so large a mass of evidence, direct and indirect, for the presence of sensations of red or green in the color-systems of the partially color-blind, Hayes thinks we should regard dichromacy as an extreme form of partial color-blindness, and class as partially color-blind, also, all mild cases of color deficiency in which an equation can be formed between an unsaturated blue-green or green and an unsaturated blue-red or red.

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HEARING

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The year 1911 has brought us no very exhaustive or systematic research in the field of hearing. Pear (7) has published some results obtained in the Würzburg laboratory which tend to substantiate experimentally the difference in degree of fusion which is usually admitted to exist between major and minor chords. He worked with both musical and unmusical observers, testing their respective abilities along this line (8) by means of the analysis of intervals, the differential limen for pitch and a "singing" test in which they were required to imitate notes given at random. He also made a "consistency" test on the basis of eleven musical intervals, varying from the octave to the minor second, by comparing each of these intervals with every other one four times.

The principal experiments were made with the Appunn tonometer. The method was that of "paired comparisons," the observer being required to judge by direct impression which of two tri-tonal chords possessed the greater degree of fusion. The extreme notes in the chords compared remained constant, and included the intervals 3 : 5, 2 : 3, 1 : 2, 2 : 5, 1 : 3, and 1 : 4. The middle tone was shifted so as to produce in each experimental comparison two chords of three constant intervals, the smaller of which was placed, now between the lower and the middle tone, now between the middle and the higher tone.

His results substantiate the assumption that "the degree of fusion of a chord varies with the position of its constituent degrees of fusion within the tonal scale, decreasing when the worse degrees are the

lower, and increasing when they are the higher." The degree of fusion "increases when the interval possessing the greater *frequency-ratio* (*i. e.*, the 'greater interval' in the musical sense) occupies the lower, and decreases when it occupies the higher position." The same is true for intervals of "greater *frequency-difference* (*i. e.*, the 'greater interval' in the physical sense)." These conclusions are in accord with the assumption that chords possessing greater *indirect* clang relationship are more highly fused than those possessing greater *direct* relationship. Meyer's tentative assumption that "the fusion of a chord of three clangs is the higher, the simpler the ratios of its frequencies, whether the chord be considered as a whole or the tones be taken in pairs" is also substantiated. Finally, the experiments seem to show that these conclusions have a more general significance than the mere differentiation of major and minor chords in music, since they apply to "unmusical" as well as to "musical" chords.

Stumpf (**12**) takes up some of the criticisms which have been urged against his explanation of consonance. After defining fusion as a *uniformity* rather than a *unity* of effect, and contrasting it with similarity, which increases with the decrease in an interval, he proceeds to warn against the correlation of fusion with physical pitch, since the pitch of a tone is known to vary with its distance from the ear. He concludes that the degree of fusion is a function of two physiological pitches, and that it is impossible for the same pair of tones, thus considered, to fuse in different degrees. Consonance and dissonance exist only between two simultaneous tones; successive tones reveal the phenomena of relationship. As long as we deal with sense impressions, consonance and dissonance exist, not in specific degrees, but in gradual differentiations. In dealing with music, however, we have passed beyond simple sense impression. Our music is based upon a tri-tonal chord, either major or minor. This chord is determined rationally by the greatest number of tones within an octave, all of which are mutually consonant in such a manner that in passing from the lower to the higher tones in succession, we pass from the stronger to the weaker degrees of consonance.

Stumpf regards the major and minor as fundamentally equal. The musical scale is built up by the derivation of successive tri-tonal chords of the sort mentioned, the so-called "dissonant" tones which thus come in are all indirectly related to the fundamental. In this system *concords* consist in any three principal tones, either major or minor, but they must contain a fifth or fourth, and a third or sixth. All remaining chords are *discordant*. Concordance and discordance are thus very much more complex than are consonance and dis-

sonance, although they are based upon these. Accordingly, the same consonant pair may be judged as concordant or discordant by virtue of the chord to which it is conceived to belong. The consonance of the pair is in no wise affected, but its concordance is. Musical thought, based upon this system of major and minor chords, is responsible, in the varying attitudes which are aroused in us, for the acceptance or rejection of single intervals. This accounts for the seeming contradiction which has been noted to exist at times between consonant pairs and musical practice.

In a critical article directed against Krüger's reply to Stumpf's original strictures upon the derivation of consonance from the effects of difference tones in the total complex, the Berlin psychologist returns (13) to his contention that the interval 800 : 1,100 ought, on Krüger's hypothesis, to be completely consonant, because no beats or mean-tones are present among the difference tones aroused. Krüger having since responded that the large number of difference tones in this and similar cases affords a complex clang which is disadvantageous to consonance, Stumpf answers that it is absurd to suppose that the mere aggregation of tones should have any such result, since no such effect is apparent in the addition of successive tones in the octave relation.

With regard to certain other "critical intervals" which Stumpf had pointed out, Krüger has contended that the "sonance" character of intervals extends only to the approximate limits of the human voice, 80–1,024 vib., beyond this, the intervals all tend to become *neutral*. This Stumpf denies, claiming that musical practice shows that a limit cannot be set under 4,000 vib. In Stumpf's opinion, then, Krüger's theory contradicts itself, even if we assume the five difference tones, the existence of which Stumpf has experimentally tested and largely disproven.[1] One consequence of Krüger's theory would be that the intervals of the third and fourth accented octaves should be the strongest and clearest in their consonant effectiveness, because here the difference tones are strongest and clearest. Yet in this range appear the "critical intervals" which in consequence of such a theory should be completely consonant, yet they are not.

Goebel (4) has made an interesting observation which he believes to be in substantiation of the Helmholtz theory of consonance. A tone of sufficient intensity, he finds, is accompanied by a second tone whose pitch is one octave lower. When two weak tones in the octave relationship are presented simultaneously, one to each ear, there appears to be no unity in the effect.[2] If, however, the higher of the

[1] Cf. "Summary on Hearing," this journal, 1911, 8, 93 ff.

[2] An analogous phenomenon has been observed by Ebbinghaus (cf. *Grundzüge*,

two tones be intensified, the unity is at once established, since the lower tone is then present in each ear. The author assumes that different cells in the same cross-section of the cochlea must be specifically sensitive to octaves. The assumption provides that the outer cells in a given cross-section may function for the higher tone, the inner cells for the lower tone, although in man, where the number of cells is small, the differential effect may be centrally produced. With the exception of very low, and perhaps also of very high, tones the author concludes that the intensification of any single tone is effective in exciting certain cells which produce an additional tone one octave lower than the objective. This may be offered in explanation of certain cases of fusion among pure tones in the octave relationship where over-tones are not objectively present to make the Helmholtz explanation of identical over-tones applicable.

In a combined report with v. Hornbostel made before the Fourth Congress for Experimental Psychology (11), Stumpf refers to the collection of phonograms of exotic music which has been in progress since 1904 at the Berlin Institute. He also indicates two interesting points in connection with the study of non-European musical systems. First, it has been found from examination of xylophones and metallophones that the Javanese have a scale of seven equal intervals, and the Siamese a scale of five equal intervals. The equality of the relations of adjacent tones is so exact that we must assume for these people a sense for equality of interval which we apparently have lost in the harmonic development of our music. Wundt's explanation that the intervals have been determined by a mechanical process of making the instrument mathematically correct as to the relative lengths of the bars of wood or metal is not substantiated by an examination of the instruments themselves. They reveal both a crude manufacture, and also filing and weighting for the evident purpose of tuning them after completion to the exact intervals required. The second point has reference to the appearance of simultaneous octaves, fifths and fourths in primitive choral singing. Stumpf assumes that this must have resulted from a selection based upon unitary effectiveness, and thus reveals the universality of the principle of fusion. He believes it probable that, prior to such selection, primitive melodies arose from the use of arbitrary small intervals which were quite unrelated.

In his portion of the report, v. Hornbostel describes another way

I., 2d ed., pp. 345–346) in substantiation of his theory of "undertones" as the explanation for tonal fusion. It may be further suggested that Goebel's observations can be quite readily adapted to the Ebbinghaus theory.

in which polyphony may have arisen. In passing from a solo to a choral part, the second party may start too soon. This would result in simultaneous tone effects some of which are perpetuated in practice because of their fundamental fusion. The usage of simultaneous major seconds, however, which he has found to be frequent, remains unexplained. The author makes a second point with regard to the very high development of rhythm in exotic music. The fact that uncivilized folk do not count the beats, as we are apt to do, probably explains their greater capacity for rhythmical groups and variations. It is interesting to study the complexity of their rhythms in combination with a melody. The rhythmic accompaniment often shows a relatively independent structure with accents which vary considerably from those of the melody.

Hermann (6) returns to the substantiation of his theory of the "formant" as the basis of vowel sounds. Experiments with the microphone method show the production of formants whose periodicity is uninfluenced by the note on which the vowel is sung. However, as soon as the vowel note exceeds noticeably the pitch of the formant, the vowel is no longer heard. The formant is therefore a fixed tone which characterizes the vowel. It is produced by a blowing process, with the mouth cavity as a resonator. This is quite different, however, from the Helmholtz notion of the resonance effect produced by this cavity in intensifying characteristic overtones. These tones would often be altogether too weak for such a purpose, as, for instance, *i* on a bass note would depend upon the 21st–29th partials. Resonators, according to Hermann, behave differently when they are blown and when they respond sympathetically. It is in the former case that vowels result from "anaperiodic" blowing of the mouth resonator in the period of the voice vibration. Whether the mouth tone is harmonic or inharmonic to the voice note is of no importance. Hermann replies briefly to Köhler's experimental attempt to disprove the formant theory, but aside from indicating that Köhler has misapprehended the nature of the relationship existing between the vowel tone and the formant, or vowel quality, he does not enter upon a critique of Köhler's interesting results.[1]

In reply to certain experiments by Fredericq which seemed to show that the speed of a phonograph has no influence upon the vowel sounds produced by it, Hermann (5) adds that the vowel character is not exclusively dependent on pitch, which, is of course, altered with varying speed of the phonograph. It depends also upon other things, as the manner in which the formant vibration is spread over the

[1] Cf. *l. c.*, 97 f.

period. For each vowel, too, the formant may vary over a certain range: *a* from e^2 to a^2, *e* from c^4 to $d^{\sharp 4}$. Certain facts are not yet explained, as the passage of high tones into *a* sounds, and the passage of *e* into *o* and *i* into *u* when the speed of the phonograph is increased. It may be that *e* and *i*, which according to Helmholtz find their characteristic in the fourth accented octave, possess also a deeper-lying formant to which they are driven by the increased pitch, and thus they approach *o* and *u* of the lower register.

Sander (9) experimented upon the effect of duration on the intensity of tones. He used two successive stimuli furnished by tuning forks and conducted to the ear by means of telephonic connection. One of these was given with a constant full intensity, but with varying durations in the different experiments. The other, by which the subjective intensity of the first was measured, had a constant duration and an easily variable intensity. Reproduction of the same stimulus, after an interval of three seconds, was marked by an increased intensity of 4 per cent. to 5 per cent. The apparent rising intensity of a tone, for all degrees of pitch and objective intensity, is rather rapid at first and then more gradual. The point of time at which the stimulus first reaches its tonal maximum cannot be exactly determined, but with the author it lies always between 615 and 925σ. The remission of intensity began for a tone of 218 vib. after 1,110σ; with the weaker tones 384 and 640, there was no remission indicated. With more intensive stimuli the tone rises more rapidly, yet it is doubtful if the maximum is reached more quickly. Increase in vibration rate plus increase in intensity effects a still quicker arousal. It is impossible to alter the quality with a constant intensity since the two factors operate together.

Urbantschitsch (14) reports a few preliminary experiments to prove that reflex movements are occasioned by sound stimuli. He had ten observers read aloud while various tones and noises were sounded. They were instructed to pay no heed to the sounds. In all cases speech was interfered with. Reflexes were called out particularly in the regions of the neck and breast. The reflexes were found to vary in different persons, but no very exact correlations were noted. The results appear to be rather slight for supporting the contention that sounds occasion definite reflex responses.

With reference to the physiology of hearing, Shambaugh (10) makes an interesting plea that more consideration be given the tectorial membrane in the functioning of the Corti organs. He points out the anatomical fact that in all three organs of the labyrinth

hair-cells are to be found in contact with a fine membrane. Whereas it is generally admitted that the stimulations of the semicircular canal and the vestibule are mediated by reciprocal effects of the hairs and the membrane above, the tectorial membrane of the cochlea is overlooked in favor of the basilar membrane. It is impossible, the author thinks, that the basilar membrane should be adequate to the function of hearing, since it is subject to a varying blood pressure which can not but affect its vibratory capacity. The reciprocal effects between the hair-cells and the tectorial membrane are not complicated by such disturbances. Although it is impossible to demonstrate the manner in which this delicate membrane might behave under stimulation, we may, perhaps, assume that different regions respond with different tones, the high tones being produced near the base, the low tones near the apex. We should thus be able to accommodate for the pathological "tone-islands," and the facts of tonal analysis.

Ewald (2) criticizes certain results supposedly in confirmation of the Helmholtz theory, obtained by Wittmaack and Yoshii. They worked (separately) on guinea-pigs, and brought about the destruction of certain regions of the Corti organ by stimulation with tones of varying pitch. Ewald shows that the regions destroyed were not definitely in accord with the theoretical location of the particular tones in question, and, furthermore, that the areas were much too large to be accommodated to the Helmholtz theory. Ewald has noticed with his "camera acustica" that small bubbles appearing on the membrane in the water are driven forward with great force by sudden intensive tones. It is in accordance with this analogy that he would explain the results of the two investigators named.

Frey (3), having demonstrated in many cases anchylosis between the malleus and incus, and furthermore, that this is in no case a true joint, concludes that there is no displacement here during the act of hearing, and that the protection of the conducting apparatus, which was supposed to be effected by the movements of these bones, may and must be explained by other factors, as the various ligaments involved.

In an interesting volume Dupré and Nathan (1) have summarized the principal results thus far obtained in the study of musical defects in various types of mental disturbance. After an introductory chapter in which the psychology of language in general, and the musical language in particular, is described in a simple, and also somewhat naive, manner, the authors proceed to the consideration of sensory, psychic and motor disturbances among aphasic,

psychopathic and insane individuals. The origin of partial amusia is, they believe, dynamic, the organic cases being invariably complex or total. They find no necessary parallelism between amusia and aphasia, and a precise cortical localization for the disturbances in musical language is not evident. In dementia musical capacity suffers along with the other mental activities, but somewhat more slowly. An interesting critical study of the psychoses of great musicians leaves little support for the contentions of Lombroso and Grasset that these individuals are peculiarly susceptible to insanity. A consideration of *melotherapy* brings the conservative conclusion that it has no great virtue as a cure for the insane. With psycho- and neuropathic cases its influence is at times undeniable, yet even here it is entirely conditional on the individual musical capacity and interests of the subject treated.

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SPECIAL REVIEWS

PSYCHOLOGY OF ADVERTISING

The Relative Merit of Advertisements. EDWARD K. STRONG. New York: The Science Press, 1911. Pp. 81.

Strong set out to compare advertisements as to "pulling power," "attention value," "persuasiveness," etc. In the main, his subjects were college students, but in certain experiments individuals from other classes of society were included. His materials were piano, soap, breakfast food and vacuum cleaner advertisements. A special study was made in the case of fifty Packer's Tar Soap advertisements.

The following are the chief results which may be gathered: An advertisement should be half picture and half copy. Direct appeals are better than indirect appeals. The strongest appeals are those which are strictly relevant and then come more general appeals to instincts and habits of life. College students represent the educated classes of the community, but do not represent the smaller towns and farming districts.

The prominence given to the purely mathematical part of the work has rather overshadowed the remainder, or, to put it otherwise, qualitative distinctions have been sacrificed to quantitative distinctions, a procedure which is always detrimental to good psychology. The comparison between two experiments by using the mean in one, and the median in the other, is not even good statistics.

The following statement occurs in connection with the Packer advertisements: "Take for granted that each advertisement represents a different make of soap." It is submitted that such inhibition is impossible except in a specially trained subject and even then not always, as the maker's name is prominent on each. Any detailed introspection is lacking, so that the very characteristic of a psychological experiment is lacking.

Taking Strong's study or procedure as typical rather than specific, attention may be drawn to some work which is being called "psychology of advertising."

Even if done in a psychological laboratory, such investigations are not psychological, for they tell us nothing about the psychological

factors which make an advertisement good or bad. It is cheerfully admitted that such a line of investigation gives many useful facts. The method used here was the rough comparison of advertisements according to "attention value," "pulling power," etc. A glance at the results shows that many of them are already known or are of such a nature as can be ascertained without any previous training in psychology.

When we compare a series of advertisements and place "reliability first" "cleanliness second," etc., as the case may be, we are not contributing to the psychology of advertising. To do that, we must ascertain why such terms take first or second place. These terms are not psychological and instead of being put down as results, they should be the objects of inquiry.

Take "attention value." Strong has given no hint as to any measure or criterion and at best it seems but the vague opinion of the observer, which in the case of the "negro elevator man" cannot be credited with the superlative of accuracy.

By a conglomeration of vague preferences, under still vaguer headings, we can never reach the psychological bases of appeal. A psychology of advertising can only be realized by a keen analysis of the conditions, not by a mere catalogue of those conditions.

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MENTAL MEASUREMENTS

The Essentials of Mental Measurement. William Brown. Cambridge: University Press, 1911. Pp. 154.

The book consists of two parts, the first dealing with psychophysics and the second with the use of the theory of correlation in psychology. Part II. is a reprint of the author's doctorate thesis and was reviewed in the January, 1911, number of the *American Journal of Psychology.* Part I. begins with a discussion of the possibility of mental measurement and the author states the reasons why he believes that such measurement is possible. Then follows a description of the different psychophysical methods. The method of constant stimuli is described in some detail and with great clearness. There is a misprint in the observation equations on p. 30, where only one side of the equations is multiplied with the coefficient of weight. Curiously enough the same misprint occurs in Titchener's *Manual*, Vol. II., Part I., p. 102. Brown gives Müller's table of weights as well as mine, which is inconsistent, since only one

of the tables can be correct. It may be mentioned in this place that many of the values in Müller's table are incorrect, since they are out by the unity or more of the last decimal place. The connection between the method of constant stimuli and the method of just perceptible difference is made by my formulæ.

The last part of the chapter on psychophysics is easily the most interesting. Brown proposes to apply Pearson's general formulæ to the study of the distribution of the threshold. This idea—though very obvious to any one acquainted with Pearson's work—is new and it ought to be tried on a large and trustworthy empirical material. Such an investigation is bound to give interesting results and it is to be hoped that the author may soon supplement his book by some such work. It is very important to see how an idea works out in practice and it also is important to know how much work the practical application of a method requires. For this purpose all the necessary calculations ought to be given in detail.

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DISCUSSION

REACTIONS TO VISUAL AND AUDITORY STIMULI

In his summary of the work of Dunlap and Wells[1] on "Reactions to Visual and Auditory Stimuli," Dr. Herbert Woodrow mentions the fact that while in the case of simple sensory reaction to visual and to auditory stimuli the former were found to be the longer, it was also found that reactions to sound and flash simultaneously presented (the reaction being to the flash, and the attention being concentrated on it exclusively during the preparatory interval) were almost as short as simple sound reactions.

Dr. Woodrow says that this circumstance naturally indicates that when the reaction was ostensibly to the flash, it was actually to the accompanying sound simply, and adds the bare statement that the authors did not accept this explanation.

In the paper in question, our reasons for not accepting the simple explanation were definitely assigned (p. 328), being based on a second set of experiments which was planned to throw light on the results of the first set. The results of this second set are fully given in the paper in question.

The reactions in the second set were with discrimination. In one group the reactions were to flash *plus* sound, flash attended to (*Fs*) discriminated from sound alone (*s*). In another group the reactions were to flash alone (*F*) discriminated from sound alone (*s*). In each series the numbers of both kinds of stimuli were equal, and the sequence in the series was determined by the order of a well-shuffled pack of cards. In these series it was found that the reaction to *Fs* was considerably shorter than the reaction to *F*, although the discrimination control rendered it out of the question that ostensible *Fs* reactions should really be *S* reactions.

While, as we pointed out in the paper, the results of these experiments are meager from the numerical point of view, and do not warrant even a provisional positive conclusion, they are of sufficient importance to prevent acceptance of the simple explanation above mentioned, and to point out a line for research.

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[1] Psychological Bulletin, 1911, 8, 387–390.

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NOTES AND NEWS

Professor John B. Watson, of the Johns Hopkins University, has recently been granted a three years' appointment as a research associate of the Carnegie Institution of Washington. In this capacity he will study the migratory and other instincts of the sea-gulls of the Tortugas, Florida.

Dr. Bird T. Baldwin, now professor of education at the University of Texas, is to have charge of the new department of psychology and education which is to be established next year at Swarthmore College.